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ABSTRACT

Using the National Longitudinal Survey of Youth (NYSY) for 1979-92, an empirical analysis documented and characterized early labor market experiences of men and women in the U.S. economy. It explored the evolution of these labor market experiences over the first 5 years in the labor market and studied the relationships between them and adult labor market outcomes. The overriding goal was to shed light on the consequences of initial periods of "churning," "floundering about," or "mobility" in the labor market, to help assess whether faster transitions to stable employment relationships would be likely to lead to better adult labor market outcomes. Findings indicated that labor market outcomes toward the end of the 5-year postschooling period used were not driven very strongly by what happened in the first year or two in the labor market. Adult labor market outcomes (in the late 20s or early to mid-30s) were for the most part unrelated to early labor market experiences, especially for men. For women, in contrast, some evidence suggested that job stability and initial entry into a high-wage occupation had beneficial effects. The evidence did not present a compelling case for efforts to target the school-to-work transition, insofar as this implied changing the structure of youth labor markets so that workers became more firmly attached tc an employer, or an industry or occupation, at younger ages. (Appendixes contain 56 references and 13 tables.) (YLB)

Econometrics and Economic Theory Paper No. 9506

ORDER FROM CHAOS?
THE EFFECTS OF EARLY LABOR MARKET EXPERIENCE ON ADULT LABOR MARKET OUTCOMES

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Abstract

This paper has three goals: 1) to document and characterize early labor market experiences of men and women in the U.S. economy, 2) to explore the evolution of these labor market experiences over the first five years in the labor market, and 3) to study the relationships between these early labor market experiences and adult labor market outcomes. The overriding goal of the empirical analysis is to shed light on the consequences of initial periods of "churning," "floundering about," or "mobility" in the labor market, to help assess whether faster transitions to stable employment relationships would be likely to lead to better adult labor market outcomes.

Our interpretation of the large set of results that we present is that there is at best modest evidence linking early job market stability to better labor market outcomes. First, we find that labor market outcomes towards the end of the five-year post-schooling period that we use are not driven very strongly by what happens in the first year or two in the labor market. Second, we find that adult labor market outcomes (defined as of the late 20s or early to mid-30s) are for the most part unrelated to early labor market experiences, especially for men. For women, in contrast, some of the evidence suggests that job stability and initial entry into a high-wage occupation has beneficial effects. Based on this evidence, which we admit is only partial, we do not see a compelling case for efforts to explicitly target the school-to-work transition, insofar as this implies changing the structure of youth labor markets so that workers become more firmly attached to an employer, or an industry or occupation, at younger ages.

ORDER FROM CHAOS?
THE EFFECTS OF EARLY LABOR MARKET EXPERIENCES ON ADULT LABOR MARKET OUTCOMES

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I. Introduction

In 1991 and 1992, President Clinton campaigned on the promise of creating an integrated system of youth education and job training to provide a faster and more successful transition from school to stable employment. A General Accounting Office report supported his claim that non-college educated youth in the U.S. tend to experience an initial period of joblessness or a series of "dead-end" jobs (GAO 1990). The integrated system was intended to address the perceived "school-to-work" problem in U.S. labor markets, helping to transform the youth labor market from the current "chaotic" system in which many youths initially experience a sequence of jobs with high turnover, to a more "orderly" system, like that of the German apprenticeship system or the informal contracts between Japanese schools and employers, in which youths leave school for further career training or stable employment.[1] In furtherance of this plan, on May 4, 1994, President Clinton signed the School-to-Work Opportunities Act into law.

The labor economics literature provides two opposing views regarding the apparent "chaotic" nature of youth labor markets in the U.S. A great deal of research documents the positive returns to training (e.g., Mincer, 1991) and job attachment (Topel, 1991).[2] The widespread acceptance of this research may make it natural to conclude that it is optimal to get workers into steady jobs with lots of training as soon as possible after leaving school; in addition, stability of jobs may spur training by workers and firms that are more likely to recoup their investments. However, there is also ample evidence that workers receive positive returns to job shopping.[3] In this case, a policy that funnels workers more quickly into long-term jobs may ultimately prove counter-productive as workers who may have otherwise found a good match with an employer no longer do so.

In this paper we seek to provide empirical evidence on the labor market experiences of youths, and their consequences for transitions to career jobs, in order to better inform this policy debate. We proceed in three steps. First, we describe numerous dimensions of youth labor market experiences, including training, wages, the stability of employment measured along a number of dimensions, the accumulation of tenure, and industry and occupation of employment. Second, we document the intertemporal relationships among these various components of youth labor market experiences, to understand the consequences, if any, of a failure to settle into a steady job or steady

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[1]See, e.g., Hamilton (1990), Lerman and Pouncy (1990), Klerman and Karoly (1995), the Commission on the Skills of the American Workforce (1990), Lynch (1993a), Glazer (1993), and other work reviewed in Heckman (1993).

[2]Alternative approaches and evidence regarding the returns to job attachment are presented in Abraham and Farber (1987) and Altonji and Shakotko (1987).

[3]See Topel and Ward (1992), McCall (1990), and Light and McGarry (1994).

employment, or a job involving training, in the first year or two after leaving school. Finally, we explore the relationship between the entire gamut of youth labor market experiences and labor market outcomes of more mature adults, to attempt to understand which types of early labor market experiences tend to lead to greater success. We look at both wages and benefits in attempting to measure adult labor market success. While there is a voluminous literature on youth labor markets and young workers, relatively little research has addressed the links between youth labor market experiences and the careers of adults, and, to the best of our knowledge, no research has attempted to take as a comprehensive a look as we do at the relationships between numerous dimensions of the youth labor market experience and adult labor market outcomes.

As is unfortunately often the case, documenting the empirical relationships described above does not necessarily provide the answers that policy makers ought to have. The evidence we compile has two important limitations. First, an implication of the job shopping hypothesis is that the cross-sectional association between adult wages and, for example, early job stability or early training, is not necessarily a good estimate of the increase in adult wages that might ensue from early job stability or training. The reason, of course, is that good matches are relatively more likely to have resulted in early job stability or training. If increases in job stability and training come at the expense of good matches, then the cross-sectional association overestimates the effect of any such policy. As a general matter, this means that we have to think carefully about how we might move to a system of more orderly labor market transitions without reducing the quality of job matches. Of more direct concern for this paper, in attempting to estimate the effects of early job training, early job stability, and other early labor market experiences, we must try to eliminate biases from the selection mechanism that partly determines which workers experience this stability or training, although our ability to do so may be limited. In recognition of the difficulties inherent in eliminating such biases, however, the estimates that we present are best interpreted as upper-bound estimates of the causal effects of early labor market experiences on adult labor market outcomes.

Second, our evidence is silent on the issue of whether there is any need for policy intervention. For the most part, the alternatives are posed as either "orderly" or "smooth" transitions to labor market careers, versus the "chaotic" transitions thought to characterize the American system, in which many youths initially experience a sequence of jobs with high turnover. A neoclassical economist is first and foremost concerned with the question of whether there is any sort of market failure that causes individuals to obtain less training or experience less job attachment than is socially

optimal.[4] There is virtually no research that addresses the market failure question,[5] and we similarly do not address it in this paper. An alternative, less ambitious question is whether more "orderly" or "smooth" transitions would boost the labor market prospects of young workers, particularly those who end up in relatively lower-wage jobs. This question ignores issues of market failure (and cost/benefit analysis) and focuses primarily on a distributional issue: namely, what can be done to raise the earnings of lower-wage workers. Our evidence speaks more strongly to this latter question, although, as indicated above, we probably obtain upper-bound estimates of any distributional effects.

II. Existing Research

Most of the existing research on youth labor market experiences focuses on short-run employment problems, or short-run effects of training, with little attention paid to the consequences of these experiences for adult career outcomes. A substantial body of research has studied the unemployment/employment experiences of young workers in various demographic groups, and their short-term effects on wages (e.g., Ellwood, 1982; Freeman, 1982). This research tends to find that there is no permanent (or even medium-term) "scarring" effect of early unemployment. The only lasting effect is that workers who experience such unemployment accumulate less labor market experience, and because of this may earn less subsequently. Other research has studied the short-term effects of labor market training, education, or apprenticeships on early labor market experiences (e.g., Blanchflower and Lynch, 1994; Glover, 1995; Lynch, 1992a and 1992b; Meyer and Wise, 1982) and on wages (e.g., Barron, et al., 1989; Bartel, 1992; Bishop, 1994; Brown, 1989; Krueger and Rouse, 1994; Lynch, 1994; Mincer, 1991).[6] This research generally concludes that education and training raises wages.

Much less research addresses the issue of job and employment stability. Klerman and Karoly (1994 and 1995) use the National Longitudinal Survey of Youth (NLSY) to document the amount of "floundering" or "milling about" in which workers engage in their early 20's. They conclude that by their early 20's, most young workers have entered a

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[4]For example, dynamic models of labor supply and human capital investment suggest that at the low wages that are typical for relatively inexperienced workers, it may be optimal to consume relatively large amounts of leisure (e.g., Ghez and Becker, 1975, and Blinder and Weiss, 1976), which may entail low job attachment.

[5]Exceptions are Bassi (1994), Parsons (1990), and Stern and Ritzen (1991), which address market failures in the provision of training.

[6]There is also some research on the determinants of the receipt of training (e.g., Altonji and Spletzer, 1991; Cameron and Heckman, 1994; Hollenbeck, 1993). However, for the most part this research does not focus on the effects of other labor market experiences--which are admittedly endogenous--on training. Altonji and Spletzer estimate the effects of skill requirements of jobs on training. Cameron and Heckman examine the effects of GED certification (relative to high school graduation) on training. Hollenbeck examines the effects of various types of postsecondary education on the receipt of training.

stable job--defined as those lasting at least one year--although high-school dropouts fare worse. However, they do not explore the consequences of different rates of transition to stable employment for later labor market outcomes. Two papers explicitly address questions relating to "dead-end" jobs. Brown (1982) uses Census of Population data to attempt to characterize jobs as "dead end," based on either within-occupation wage growth or the probability that workers in an occupation remain in the same industry (over a five-year period). He concludes that the industry retention rate is significantly negatively related to unemployment, so that "dead-end" jobs--i.e., those with low retention rates--are associated with a higher likelihood of unemployment. However, the results for the wage-growth measure are ambiguous. Lynch (1993b) studies labor market experiences in entry-level jobs, finding some evidence that on-the-job training decreases the probability of leaving one's first employer, while off-the-job training increases it (as theory would predict), but that only the latter result is statistically significant, and only for women. However, Lynch's work is more limited than ours because she uses NLSY data only through age 25, and therefore does not study the consequences of early labor market experiences for later career outcomes.

Evidence on the links between youth labor market experiences and later careers is more scant, and generally more recent. Most of this research focuses exclusively on training. Wholey (1990) looks at the effects of training on job tenure and mobility, using a sample drawn from a survey of manufacturing establishments. He finds that training tends to increase job tenure, but does not focus particular attention on the strength of this relationship for young workers. Adams and Mangum (1986) address the relationship between training and wages and employment at ages 24-34, using the National Longitudinal Survey of Young Men, finding that company training offered much larger wage gains than training by business and technical institutes, and that managerial training boosted later wages and employment probabilities. However, the empirical analysis is limited to the effects of training, and ignores other dimensions of early labor market experiences. Also, it refers to the longest and most recent training period, rather than training in the early years in the labor market. Lillard and Tan (1986) use data from the various NLS cohorts to document some longer-term effects of training, showing that company training has positive effects on wages that last for about 11 years, and that vocational training is associated with less likelihood of unemployment, an effect that lasts for about 12 years. However, they do not pay attention to self-selection problems with regard to who receives training, and do not look at dimensions of early labor market experiences other than training. Murnane, et al. (1994) also look at some of the longer-run effects of training using the NLSY, but only for high-school dropouts.

Like Klerman and Karoly, Light and McGarry (1994) also present evidence on the evolution of job stability

among young workers. In addition, though, they examine the consequences of job stability or mobility for wages, finding that early mobility is associated with higher wage growth, consistent with job matching. On the other hand, mobility that occurs after the first two years in the labor market tends to be associated with lower wage growth, even controlling (albeit with weak identification) for individual-specific effects. The most detailed study of youth labor market experiences and their longer-term consequences is Gritz and MaCurdy (1992), who specify a Markov transition model for five possible states: low-wage employment, high-wage employment, combined low-wage and high-wage employment, training, and nonemployment. Their research is not directed explicitly toward the issues that we consider, but a number of their results pertain to these issues. For example, they find: 1) that there is a great deal of mobility out of low-wage jobs and into high-wage jobs, and relatively little mobility in the other direction, 2) that low-wage jobs are held for relatively short periods, and 3) that training in the early years in the labor market is associated with only marginal increases in employment. We examine a much wider set of early labor market experiences, and a broader array of adult labor market outcomes.

III. The Data and Sample

We utilize the NLSY for the years 1979-1992. The NLSY provides the information required to identify and to describe in detail the alternative career paths of youths for a period of 13 years, and as a result offers unique advantages for this study. Comprehensive schooling, labor market, and training variables are collected each year, with the yearly data supplemented by information from the continuous work history file on variables such as weeks and hours employed and tenure with employers. The training questions asked in the NLSY can be grouped into three broad categories: on-the-job training, off-the-job training, and apprenticeships, with complete information offered for up to three spells of training per year. Also, the NLSY has test scores for most respondents from the Armed Services Vocational Aptitude Battery Test, which we use to attempt to reduce biases from ability differences across workers that may be correlated with both wages and early labor market experiences (such as the receipt of training).

The sample used in this study is first restricted to individuals who were neither in the military subsample nor reported any military duty through 1992. This reduces the available sample to 10,716 individuals. Next, we eliminated any individuals with non-interviews between 1979 and 1986 (for reasons described below), reducing the sample to 9,121. These restrictions are relatively standard.

The next set of restrictions imposed on the sample are required in order to focus on individuals' first years in the labor market. We chose a window of five years, believing that this window would be sufficiently long to observe

many individuals' transitions from their earliest entrance into the labor market into steadier employment, possibly accompanied by training. The tradeoff is that the longer the window we use, the smaller the sample gets, as it becomes more likely that we get a non-interview, or that, as described below, some of the questions change and become unusable. Using a period of only two or three years, which would give us a larger sample, seemed unwise, given evidence both from survey data and interviews that youths typically spend their first year or two in the labor market in pursuits which are not strongly related to their careers, after which they start to make labor market decisions with career goals in mind (Osterman, 1980).[7] One constraint that is imposed is that the training questions were asked on a consistent basis through 1986, after which they changed and became non-comparable; thus, we cannot look at five-year windows that end after 1986.

Finally, we want to attempt to study individuals after they enter the labor market. However, this is an ambiguous concept, since some individuals acquire work experience during school (e.g., Griliches, 1980), and others go back to school after working. A natural procedure is to count entry into the labor market from the first year in which individuals were observed out of school. However, because successful school-to-work transitions may entail course work at community or other two-year colleges, we did not necessarily want to restrict our analysis to the period after individuals report no additional schooling. Thus, we chose to date entrance into the labor market as the first year in which individuals no longer report schooling other than that which occurred at two-year colleges (through 1992, or the year before they attrit).[8] This leaves us free to study community college enrollment in the same way that we study training, for example. Because we wish to study characteristics of early jobs, respondents who were not observed in a job at any time during the five-year post-schooling period were dropped from the sample. Other than that, there is no lower limit on the amount of time they had to have worked during that period.

The set of restrictions that we impose to obtain a five-year window on individuals' first entry into the labor market reduces the sample substantially, to 3,039. We lose an additional 108 observations with missing or inconsistent data on training spells, and an additional 400 or so observations owing to missing data on other variables, leaving a

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[7]Osterman refers to this initial period as a "moratorium" period, "a period in which adventure seeking, sex, and peer group activities are all more important than work" (p. 16).

[8]The NLSY reports enrollment at two- or four-year colleges. Researchers using the NLSY use "two-year college" and "community college" interchangeably (e.g., Klassen, 1990), as do researchers using other data sets (e.g., Rouse, 1995; Monk-Turner, 1983).

final sample of 2,537 for the analysis of early labor market experiences.[9] However, given the constraints imposed by the data set, there is no way to avoid this shrinking of the sample. This obviously raises the issue of sample selection bias. To a large extent, whether or not respondents to the NLSY are in our sample depends on their age. Individuals who are too young may not get into the sample because they do not accumulate five (potential) years in the labor market by 1986, while individuals who are too old may have entered before 1979, in which case we do not have sufficient information from the pre-1979 years to include them in the sample. However, inclusion in the sample will also be affected by schooling decisions, which affect the timing of labor market entry as we measure it. In particular, we are less likely to include in the sample older members of the NLSY cohort who get little schooling, and younger members who get lots of schooling. This type of sample selection may bias estimates of the return to schooling. For example, if those who get the highest wage offers tend to leave school, then the estimated returns to schooling are likely to be biased downward (see Griliches, 1977). However, determining whether this selection biases the estimates of the effects of other indicators of youth labor market experiences is beyond the scope of this paper.

When we analyze adult labor market outcomes, we require data on the outcomes of interest for at least one of the years in the 1990-1992 period, as well as valid data on the standard ingredients of wage regressions. This reduces the sample size for this analysis to 2,153, and the sample sizes fall more when we study benefits, primarily because of unavailable data for self-employed and part-time workers.[10]

IV. Labor Market Experiences in the First Five Post-Schooling Years

In this section we provide descriptive information on labor market experiences in the first five years after leaving school. We provide histograms to describe the distribution of some of the variables and information on the evolution of early labor market experiences over the five-year post-schooling period. Descriptive statistics are provided in Appendix Tables A1 and A2.[11]

The first panel of Figure 1 shows the cumulative distribution of spells of community college enrollment as of

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[9]In this sample, the average age of individuals is 19.9 as of the first post-schooling observation, and 24.9 as of the fifth.

[10]The average age of individuals in the sample used for the wage regressions is 30.9.

[11]To correct for the NLSY's oversampling of minority and low-income households, we adjust the NLSY weights to reflect differences in the probability of remaining in the sample given our rules for selection into the sample. We adjust these weights based on race, sex, and education (four categories). All descriptive statistics and histograms are based on weighted data.

the end of the five-year post-schooling period.[12] As of the fifth year, about 12 percent of respondents report one year in which they were enrolled in community college, and a very small number report two or more such years.

The remaining panels of Figure 1 show the distribution of training spells at the end of the five-year post-schooling period.[13] As the upper right-hand panel shows, by the fifth year (also reported in column (2) of Appendix Table A1), 26.5 percent had some type of training during this period. Of these, most had only one spell of training, and only a handful had more than three spells of training.

We computed similar statistics for training disaggregated into three categories: 1) on-the-job-training, 2) off-the-job training, and 3) apprenticeships.[14] Each worker may experience more than one type of training in the five-year post-schooling period. The lower left-hand panel of Figure 1 displays the histograms for spells of on-the-job training. A relatively low number of respondents report one or more spells of on-the-job training any time over the five-year period. Off-the-job training is by far the most prevalent type of training reported in the NLSY, as indicated by the fact that the lower right-hand panel of Figure 1 mirrors the histogram for any training.[15] About 15 percent report at least one spell of off-the-job training by the end of the fifth year, and virtually none report more than two spells.

We are also interested in the duration of training.[16] In the left-hand panel of Figure 2 we provide information on cumulative weeks of on-the-job training spells, for the individuals in the sample who reported at least one spell of

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[12]In the histograms, each shaded bar depicts the proportion with the value shown at the lower-left corner of the shaded bar.

[13]From 1979 to 1986, respondents are questioned regarding three spells of training which lasted at least one month; because of the restriction to spells lasting at least one month, we probably underestimate training relative to other data sources, such as the CPS supplements. For each training program, information is elicited on the starting date, the ending date, hours per week attended, weeks per training spell, and type of program. For the first two types of training listed by the respondent, follow-up questions on the ending date and length of program are asked if the respondents had not completed the program at the date of the last interview. Because training data were not collected in 1987, no ending date information is given for the (up to) three training spells reported in progress as of the 1986 interview. Consequently, whenever there is a training spell that has not ended by the interview date, the interview date is used as the date of completion for that program.

[14]On-the-job training undoubtedly understates informal training (see Loewenstein and Spletzer, 1994). Off-the-job training is training obtained at barber or beauty schools, business schools or colleges, vocational or technical institutes, nurses' programs, flight schools, or through correspondence courses. We do not report descriptive statistics or histograms for apprenticeship training, since the fraction of respondents with apprenticeships is very low (less than one percent).

[15]This is also reflected in the descriptive statistics in the appendix tables, which show that only 7.9 percent of respondents report on-the-job training, whereas 19.4 percent report off-the-job training.

[16]Effective weeks of training variables are constructed using the variables measuring weeks of training and hours per week of training, to create a total hours of training variable, which is then divided by 40. Thus the duration of training variable measures weeks of training, based on a 40-hour week.

training.[17] In the fifth post-schooling year, a bimodal distribution of the duration of training is apparent, with just under ten percent of the sample reporting 50 or more weeks of training, and about 20 percent of the sample reporting four or less weeks of training. The right-hand panel of Figure 2 reports similar information for off-the-job training. Paralleling on-the-job training, a sizable percentage (just over ten percent) of respondents receives more than 50 weeks of off-the-job training in the five-year post-schooling period, while there is also a significant clustering of observations with fewer than ten weeks of such training.

We next turn to a description of the amounts of work experience accumulated in the five-year post-schooling period.[18] Figure 3 shows the cumulative distribution of months of employment in the five-year post-schooling period. For each year, we truncate the distribution at ten months multiplied by the number of years, reflecting the number of months associated with nearly full-year employment. In every year, at least 50 percent of the sample had acquired ten or more months of work experience per year, reflecting nearly full-year employment for each year. This percentage rises slightly as we move further into the post-schooling period, consistent with some individuals moving from intermittent (or no) employment to full-year employment. The part of the sample that does not work full-year in each year is relatively uniformly distributed across the range from zero to one less than the maximum number of weeks.

We next look at the duration and stability of jobs held, rather than the stability of overall employment. Figure 4 reports the distribution of tenure on the longest job held in the five-year post-schooling period, for each year.[19] For each histogram, we truncate the distribution at the maximum number of quarters that an individual could have worked, which causes this distribution to shift to the right in each successive year. At the end of the five-year period, the modal longest job tenure is nine quarters, or just over two years. Also, as we would expect, the proportion of longest jobs with relatively low tenure drops as we move through the post-schooling period--although this proportion remains

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[17]While durations of training spells here are translated into full-time weeks of training, it is important to remember that the training spells detected in the NLSY are those that lasted at least one month, without regard to the hours of training. Thus, cumulative hours from a spell of training can still be quite low if, for example, a training program consists of a small number of hours per week.

[18]The NLSY provides information on up to five jobs per year for each respondent, one of which is also identified as the "current or most recent job." The information for the (up to) four additional jobs per year only covers those jobs in which the respondent worked more than 20 hours per week and for at least nine consecutive weeks. As a result, this information may give a non-representative sample of the types of jobs held in the early years in the labor market. The current or most recent job, in contrast, is not restricted to such jobs, and as a result provides a random sample of the jobs held, although at the cost of obtaining a less complete work history. Consequently, we use information on the current or most recent job held in each year.

[19]Some individuals indicated that they began a job before the beginning of the post-schooling period. We consider such jobs, but only tenure accumulated since the beginning of this period.

relatively high. For example, as shown in Figure 4, by the fifth year less than 15 percent of the longest tenures were four quarters or less, and less than 35 percent were eight quarters or less.

Figure 5 documents the distribution of first and last jobs (in the five-year post-schooling period) across one-digit industries and occupations. Looking first at industry, by far the largest proportion of first jobs (29.1 percent) are in wholesale and retail trade, a proportion which declines to 23.3 percent by the end of the five-year post-schooling period. The proportions in jobs in the goods-producing sector (mining, construction, and durables and nondurables manufacturing) rise over the post-schooling period, from 27.1 percent to 29.8 percent. For the other service-producing industries, the proportion of jobs rises in some cases and falls in others. Turning to occupations, the largest proportions work in clerical and service occupations (21.8 percent and 18.6 percent, respectively) on their first jobs, and the proportions in these occupations fall over the five-year period. The next most frequent occupation for the first job is operatives (10.1 percent), the proportion in which also falls. On the other hand, the proportions in managerial and professional jobs and craft occupations grow considerably over the five-year period (by 6.5 and 3.8 percentage points, respectively). Thus, the youth labor market experience frequently begins in clerical or service occupations, and in the wholesale and retail trade industry. However, during the course of the five-year post-schooling period, there is movement into higher-paying, more-skilled industries and occupations.

V. The Importance of the Earliest Labor Market Experiences

In this section we attempt to describe how the evolution of the youth labor market experience depends (if it does) on characteristics of the earliest jobs a person holds. For example, we saw that a sizable percentage of individuals in the sample settle into relatively stable jobs. We are interested, in this section, in assessing the extent to which this appears to depend on the stability of the earliest jobs. Similarly, we explore the extent to which training receipt in the earliest years is related to training receipt in later years.

The goal of this section is not to consider the full set of determinants of characteristics of early jobs. Rather, it is simply to provide some idea of the consequences of early job stability, training, etc., for the same characteristics of employment and training a year or two later. At the same time, we caution against a causal interpretation of the findings reported in this section, since we cannot fully eliminate the influences of individual heterogeneity that might lead to exacerbation of the apparent influence of past outcomes on future outcomes.

Training

Table 1 summarizes the empirical relationships among training in the first three post-schooling years.[20] For year 1 (the first year in the five-year post-schooling period), 12 percent reported training. The table reports the probability of reporting an additional zero, one, or two years with training, depending on whether or not one received training in the first year. Of those who received training in the first year, the probability of no additional years with training is .77, the probability of one additional year is .21, and the probability of two additional years is .02. In contrast, of those who did not receive training in the first year, the probability of no additional years of training is higher (.90), while the probabilities of an additional one or two years are lower (.09 and .01, respectively). Thus, those who do not receive training in the first year are relatively less likely to report additional years of training.

Of course, the differences between the probabilities of reporting any particular number of years with training (as opposed to any particular number of additional years) are even greater. We can see this by comparing a probability in the first row with the probability in the second row that is one entry to the right, since those in the first row already report one year of training. For example, the probability that an individual reporting training in the first year ultimately reports two years with training is .21, while the corresponding probability for those with no training in the first year is .01. The probability that the former individual ultimately reports one year of training is .77, vs. .09 for the individual with no training in the first year. Thus, training in the first year is quite important, in that, at least in the first three years, those who do not get such training are much less likely to report the same number of years with training, and are also much less likely to report the same number of incremental years with training. Of course, this conclusion must be tempered by the qualification that we are not necessarily looking at causal effects of early training on late training, but are most likely also picking up effects of individual heterogeneity with respect to who receives training.[21] If the findings do not solely reflect individual heterogeneity, however, they suggest that there are some advantages conferred by job training immediately after leaving school, or conversely that workers who do not get such training may have some trouble making up ground.

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[20]Because of the relatively low incidence of training, many of the cell sizes become very small by the third year; we therefore do not extend these calculations for the full five years.

[21]The cell sizes were too small to draw meaningful inferences from a similar analysis for on-the-job training, given the relatively low incidence of on-the-job training. Given that off-the-job training constitutes most of the training detected in the NLSY, the results were quite similar when we looked at this type of training in isolation. Those who do not receive training in the first post-schooling year are less likely to receive training in the second year (4 percent vs. 11 percent). In addition, among those with no training in the first year, only about 8 percent received training in later years, while among those with training in the first year, 19 percent received additional training.

Work Experience

Table 2 provides a similar analysis for the accumulation of work experience, based on whether or not an individual accumulated nine or more months of experience in each year. In Panel A of Table 2, we classify individuals by whether they worked full-year in the first post-schooling year, and report the probabilities of an additional zero through four years of full-year work. The first column in the table shows that those who did not work full-year in their first year have a higher probability (.19 vs. .03) of no additional years of full-year work, and a much lower probability (.33 vs. .63) of an additional four years of full-year work. However, the probabilities for an additional one, two, or three years of full-year work are relatively similar, and in two out of three cases higher for those without full-year work in the first year. Thus, while individuals who do not work full-year in the first year are less likely to convert to four years of full-year work, they are quite likely to end up with one to three such years, although also more likely to end up with no additional years of work.

More importantly, the calculations in Panel A do not reveal how one's probability of full-year work in future years change after switching to full-year work. Panel B of Table 2 provides summary information with respect to those who switch to full-year work in the second post-schooling year, by classifying individuals who worked full-year in years 1 and 2, full-year in year 2 only, and full-year in neither year. This panel shows that once one converts to full-year work there is little difference with respect to the probability of accumulating additional years of full-year work. For example, the probability of accumulating an additional three such years is .75 for those who worked full-year in years one and two, and .64 for those who did not work full-year in year 1, but switched to doing so in year 2.[22] The probabilities are also similar for the other numbers of accumulated years of full-year work. In contrast, those who do not work full-year in either of the first two years have much a much lower probability of accumulating an additional three years of full-year work. Thus, once an individual exhibits a period of strong labor market attachment, an earlier period of weak labor market attachment appears to have relatively small consequences for subsequent labor market

[22]We also computed these probabilities for those who worked full-year in the first year but not the second. These probabilities were .16 (for no additional years), .20 (for one year), .22 (for two years), and .42 (for three years). Thus, there is a substantial difference in the probability of observing permanent full-year work for those who worked full-year in year 2 but not year 1, compared with those who worked full-year in year 1 but not year 2. These results contrast, to some extent, with those that Gritz and MaCurdy (1992) obtain for men, from a more sophisticated analysis using the continuous work history files of the NLSY, although there are numerous differences in the sample used, the definitions of variables, etc. They conclude that "the total amount of previous labor market experience is a prominent predictor of individuals' future employment, irrespective of whether the previous experience is recent or not" (p. 6-35). However, they do find that more recent experience tends to be associated with a slightly higher probability of high-wage employment in the future.

attachment in the early labor market years.

What can we conclude? Individual heterogeneity probably biases upward our estimates of the effect of early experience on later experience. But even if the entire effect is causal (i.e., there is no heterogeneity bias), the numbers in Table 2 suggest that weak labor force attachment in the first year or two is relatively unimportant. Once a person works relatively continuously for a year, the probability of doing so in subsequent years is not much less than the probability for those who worked continuously all along. Alternatively, a year or two of "milling about" does not appear harmful.

Job Tenure

Table 3 reports results from a similar analysis for job tenure, classifying workers as to whether or not they worked continuously for the same employer (for four or more quarters, rounded) in each year. Panel A reports the probabilities of an additional one through four years of employment at long-tenure jobs, for workers who did and did not have a long-tenure job in their first year. Those who did not have a long-tenure job in their first post-schooling year are much less likely to have long-tenure jobs in each of the following four years (a probability of .30 vs. .66), and much more likely to have no long-tenure jobs in the subsequent four years (.20 vs. .02). However, once an individual converts to a long-tenure job, the importance of a previous year without a long-tenure job appears to be greatly mitigated, as Panel B shows. Of those with long-tenure jobs in years 1 and 2, the probability of long-tenure jobs in the next three years is .79, vs. .74 for those with a long-tenure job only in year 2. On the other hand, the probability of no additional years of long-tenure jobs is slightly higher for the latter group (.04 vs. .03). Recognizing that, if anything, the figures in Panel B probably overstate the causal effects of initial weak attachment to an employer on subsequent attachment, we conclude that earlier weak attachment to employers has relatively little lasting effect.

A Multivariate Analysis of Labor Market and Job Attachment

Next, we present a multivariate analysis of labor market and job attachment that permits more precise statements regarding whether (and how much) attachment in the earliest years affects later attachment, and permits us to assess--albeit to a limited extent--the influence of heterogeneity on estimates of the effects of past labor market or job attachment. Table 4 reports estimates of a probit model for whether an individual worked nine or more months ("full-year") in the fifth post-schooling year. In column (1), the only independent variable is whether the individual worked full-year in the first post-schooling year, as well as dummy variables for the year from which the observation was drawn (to control for aggregate labor market effects). Of course, the dummy variable for full-year employment in the

first year is not truly exogenous if there is heterogeneity in the propensity to work full-year that is correlated across the years. The estimate reveals that the probability of working full-year in year 5 is .27 higher for individuals who worked full-year in year 1. In column (2), we instead add controls for full-year work in each of the first four years. The estimated coefficients of each of these variables are statistically significant. However, the estimates for work status in years 1-3 range from .07 to .14, while the estimate for work status in year 4 is .47. Once individuals switch to full-year work, they are very likely to continue with full-year work, irrespective of what they did in earlier years. Nonetheless, even controlling for year 4 work status, the earlier history does matter. To quantify this, note that the estimates in column (2) imply that, relative to someone who did not work full-year in any of years 1-4, someone who did not work full-year in years 1-3 but d· o in year 4 has a .47 greater probability of working full-year in year 5, compared with a .58 greater probability for someone who worked full-year only in years 3 and 4, and a .79 greater probability for someone who worked full-year in years 1-4.

Of course the results in column (2) may simply reflect heterogeneity bias. Thus, early work status may appear to matter because, for example, the person who worked in yeais 1-4 may simply be more likely to work in any year, rather than because early full-year work influences the probability of later full-year work. To the extent that this heterogeneity is persistent across the years, the estimates in column (1) should reveal the maximum bias that this heterogeneity introduces, since the estimates in that column document the correlation between year 1 and year 5 work status. Thus, the fact that the cumulative effects documented in column (2) are larger than .27 suggests that past work attachment actually effects current work attachment.[23] To shed further light on the role of heterogeneity bias, in column (3) we add a set of controls for individual characteristics. One of these is a residual ability measure from the Armed Forces Qualifying Test (AFQT). The AFQT score is formed from four tests given as part of the Armed Service Vocational Battery Test, given in 1980 to NLSY respondents. To standardize by age, we regressed the AFQT score on single-year age dummy variables, and use the residuals here.[24] If the estimates in column (2) reflect heterogeneity bias, then we would expect the estimated relationship between past and current work status to be partly mitigated when we control for a wide variety of individual characteristics. As the estimates reveal, however, this

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[23]Alternatively, a higher number o· ırs of full-year work may simply be a proxy for a higher propensity to work. For a more formal tre .ent of this heterogeneity vs. state dependence problem see Heckman (1983).

[24]A similar procedure was followed in Blackburn and Neumark (1993). That paper provides a discussion of the rationale for this approach, and alternative approaches.

estimated relationship is hardly affected.[25]

Finally, in column (4) we verify that the relationships considered so far are not unique to year 5. We do this by estimating the model for full-year work status in year 4, including controls for work status in years 1-3. The same qualitative conclusion emerges; the previous year's work status is by far the most important predictor of the current year's work status.

Table 5 reports results from a similar analysis for the probability of accumulating four quarters of tenure with a single employer during a year. The qualitative conclusions for this outcome are very similar to those for general labor market attachment. Specifically, attachment to an employer is strongly related to attachment to an employer in the previous year, and once we control for the previous year's attachment, earlier attachment is not a very important predictor of current attachment. In fact, in each of columns (2)-(4), the estimated coefficient of the previous year's employer attachment variable is statistically insignificant, so for job tenure, an initial year or two of weak attachment is inconsequential.

VI. The Impact of Early Labor Market Experiences on Adult Labor Market Outcomes

We now move on to assess the impact of early labor market experiences on adult labor market outcomes. Specifically, in this section we estimate the relationship between early labor market experiences and wages and benefits, for jobs held in or near 1992, after controlling for the usual human capital and other control variables.[26] Note that even though we are using labor market outcomes at relatively young ages for some workers, these workers are all well beyond the five-year post-schooling period we studied in the previous sections, because this period had to end by 1986.

Wages

We focus most of our attention on wages, presuming that wages are perhaps the single most useful indicator of labor market success. We estimate standard log wage regressions separately by sex, and with and without including tenure and its square, but adding in the early labor market measures. Table 6 reports results from a multitude of wage

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[25]The same was true when we restricted attention to more unambiguously "exogenous" variables such as race, sex, and ability.

[26]To boost the sample size, if an observation working with a wage was not available in 1992, we tried to obtain one first in 1991, and then in 1990. The regressions all include years of education, actual experience and its square, and dummy variables for currently married, non-white, residence in an SMSA, regions, and the year of the observation. Because the returns to early labor market experiences may be realized partly through the eventual industry, occupation, and union status of employment, we do not include the corresponding dummy variables. The estimated coefficients of the usual control variables were standard.

regressions estimated for men.[27] In columns (1)-(5) sets of variables corresponding to various dimensions of youth labor market experiences are added one at a time, with the regression estimated separately for each set of variables. Thus, for example, the number in the upper left-hand corner is the estimated coefficient of the number of training spells when that variable (alone) is added to the base wage regression from the previous table. The three estimates below that are the estimated coefficients on variables for the numbers of each type of training spell, when those variables (and no others) are added to the base wage regression. Finally, the last rows of the table indicate other variations in the specification, which will be explained below. Then, in columns (6) and (7) we report results when we include the set of variables measuring the early labor market experiences jointly. The specifications in columns (1)-(7) exclude tenure and its square. Columns (1') and (7') report estimates of the specifications from columns (1) and (7) after adding tenure and its square. We were concerned that current tenure would also pick up some of the effects of early labor market experiences, and therefore wanted to look at specifications that do not "over control" by including tenure. On the other hand, we do want to know whether early labor market experiences matter, once we account for "adult" tenure. Table 7 reports estimates of the same specifications for women.

Considering each set of variables independently is useful because it permits us to identify the "gross" effects of a particular set of early labor market experiences. For example, our results consistently suggest that training increases wages. But if we simultaneously control for training and early stability, and find that stability does not matter, one might object that stability matters, because it leads to more training. By looking at regressions in which we include the stability variables only, we can test this proposition, because in such regressions the effects on training should "load" onto the stability variables.

We first consider the estimated impact of training in the early labor market years. The first specification, in column (1) of Table 6, indicates that each spell of training boosts adult male wages by eight percent, an effect that is statistically significant. Below this estimate, we report coefficients when we simultaneously include separate variables for spells of each type of training. The estimated coefficient is positive for each type of training, although insignificant for on-the-job training. This last finding differs from the literature surveyed earlier that studies the effects of training on wages of young workers, in which on-the-job training is associated with higher wages; the difference may arise because the later dates at which we measure wages imply that many individuals have changed jobs, thus losing any

[27]Throughout the following tables, we report enough digits for the reader to accurately assess the statistical significance of the estimated coefficients. For example, when we report an estimated coefficient of .02, and a standard error of .01, the actual estimated coefficient was statistically significant at the five-percent level.

firm-specific human capital, or it may reflect the finding from Lillard and Tan (1986) that the effects of training diminish over time. Below these estimates, we look instead at accumulated full-time weeks of training, converted to years for the wage regressions, to facilitate comparisons between, for example, years of schooling and years of training. Whether we look at all training combined, or each type of training considered separately, the estimated effects are all statistically insignificant.

Part of the reason we find positive effects of earlier spells of training may be not because training actually increases wages, but because higher-wage individuals get training. We approach this question a couple of ways. First, we try to control for omitted ability by including the AFQT (residual) score. The estimated coefficient of the AFQT score (not reported) is positive and statistically significant in all specifications, for both men and women. When the AFQT score is added, in column (2) of Table 6, the estimated effects of the training variables all become smaller. However, the estimated coefficients for off-the-job and apprenticeship training remain positive and statistically significant.[28]

Next, we try to control for individual heterogeneity more completely by including the first wage in the five-year post-schooling period in column (3), and then (alternatively) the last wage in column (4). To standardize, we regressed the raw log wages on dummy variables for the years from which the wages were drawn, and use the residuals here. The estimated coefficients of these early wage variables (not reported) were always positive and significant at the five-percent or ten-percent level. The inclusion of these early wages has little effect on the estimated coefficients of the training variables. The only exception is that when the last wage is included, the estimated coefficient on spells of apprenticeship training falls and is no longer significant. We caution, however, that inferences regarding apprenticeship training are based on relatively few observations.

In column (5), we return to the problem discussed in the Introduction of separating out the effect of early labor market experiences (training, job tenure, etc.) from the effect of quality of the job match. If training is associated with good job matches (which are in turn associated with higher wages), then we would not want to interpret the estimates in columns (1)-(4) as measuring the causal effect of training. (The same will be true, in an even more obvious way, when we try to estimate the effects of job attachment during the early years in the labor market.) To solve this problem, we

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[28]This parallels other research indicating that the cross-sectional association between wages and training is for the most part not attributable to individual heterogeneity (Bartel, 1992; Brown, 1989; Holzer, 1990; Lynch, 1992b; Mincer, 1991). Below, we also report within-family estimates that constitute another attempt to remove heterogeneity bias.

estimate the same regression only for those individuals who are not with the same employer for whom they worked at the end of the five-year post-schooling period over which the early labor market experiences are measured. This will probably lead to some understatement of the effects of early labor market experiences, because we eliminate not just the effect of the quality of the job match, but the effect of any returns to early labor market experiences (in this case training) that are specific to the employer, or that arise via greater stability.

The changes in the estimated coefficients of the training variables are, surprisingly, not very marked. The estimated coefficient for any training spells actually rises slightly (to .07), suggesting that the training is not specific. Similarly, the estimated coefficient of off-the-job training is actually slightly higher, consistent with this training being largely general. The estimated effect of spells of apprenticeship training becomes even smaller for this sample.

Next, in columns (6) and (7) we report results for the column (1) and column (5) specifications, including all of the early labor market variables simultaneously. However, we do not distinguish types of training. In these specifications, the effects of training are somewhat larger, as those reporting at least one spell of training earn wages that are higher by nine to 11 percent. Finally, columns (1') and (7') report estimates of the same specifications, with the addition of tenure and its square to the wage regressions. The results are unchanged.

Table 7 reports estimates of the same specifications for women. The differences between the results for men and women can be summarized simply. For women, in contrast to men, on-the-job training has strong positive effects on wages, while off-the-job training does not. This is true whether we look at discrete indicators of any spells of training, or at duration of training. The magnitude of the effect of length of on-the-job training is perhaps implausibly large. It is worth bearing in mind, though, that most training programs are much shorter than a year, so that most women would capture far less than the 26 percent gain for a one unit increase that is implied by the estimate in column (1). The results are the same when we add in the AFQT score, or early or late wages, or when we restrict the sample to those not on the same job as at the end of the five-year post-schooling period. This last result is most surprising; because the sample is restricted to women who are not at the same employer as in the immediate post-schooling period, the result implies that on-the-job training that women receive in their early labor market years is general, rather than specific.

The next set of specifications includes the number of spells of community college, which may be regarded as an alternative type of training. For men, in Table 6, the estimated effect of community college is always negative, but never significant. Recall that the wage regressions also include years of schooling, so what these coefficients estimate

is the returns to community college above and beyond the usual returns to schooling. For women, in Table 7, the estimated effects of community college are positive, but also insignificant. Thus, there is no statistical evidence that community college is a particularly effective method of boosting adult wages (relative to other schooling).

In the following row of the table, we report specifications including longest tenure attained during the five-year post-schooling period in the wage regressions. For men, in Table 6, the estimated effect is positive and significant in all of the specifications estimated for the full sample (columns (1)-(4) and (6)), which exclude tenure. The estimates indicate that an additional year of early job tenure is associated with adult wages that are higher by two to five percent. However, in column (5) and (7), when we restrict the sample to those not working at the same job as at the end of the post-schooling period, the estimate falls and becomes insignificant. Thus, the return from early job attachment does not carry over to other jobs. We caution against interpreting this result too strongly, as implying that there are no real returns to early job attachment. There may be returns that persist as long as one stays on the same job. But we cannot distinguish these returns from the higher wage attributable to match quality, and hence do not want to conclude that the estimated effects in columns (1)-(4) are effects that could be captured in the absence of a good job match. Reflecting a similar concern, in column (1'), when we add tenure and its square to the wage equations, the estimated effect of early job tenure falls by half although remaining significant. (However, in all of the specifications corresponding to columns (2)-(5), when tenure was included, the estimated coefficient of early job tenure was .01 and insignificant.) This, again, suggests that the only individuals who appear to earn higher wages as a result of early job stability are those who have accumulated a lot of tenure, and have probably remained on the same job. Greater job stability in the early years, in and of itself, has no independent, lasting effect on adult wages, although the differences between the estimates excluding and including tenure imply (not too surprisingly) that early job stability is positively related to tenure as an adult. For women, in Table 7, the effect of longest job tenure in the five-year post-schooling period is generally positive, but is smaller and statistically insignificant.

We next attempt to estimate the effects of job instability or churning in a somewhat different manner, by looking at wage differences based on whether a respondent continues to work in the same one-digit industry or occupation as their first job. For men, the results in Table 9 suggest that the effects of remaining in the same industry or occupation are if anything negative. This may reflect the fact that young workers tend to begin employment in lower-wage industries and occupations, so that those left in these industries and occupations eventually earn less. These results suggest that reducing "milling about" in the labor market by locking young workers into the industries and

occupations in which they first find employment might be the worst of all possible worlds. For women, in contrast, neither variable has a significant effect on wages, whether or not we control for tenure.

Rather than looking at whether an individual remains in the same industry or occupation, we next ask whether the wage level of an individual's first industry or occupation has any effect on adult wages. In particular, we index industries and occupations by the average starting wage paid to individuals in the sample. Again, these wages were first regressed on dummy variables for the years from which they were drawn, to remove any effects of inflation. The results can be summarized succinctly. There is no significant relationship between the wage in the initial industry or occupation and the adult wage, for any of the specifications or subsamples. Thus, starting in a high-wage industry or occupation appears to yield no lasting benefits in terms of wages.

Finally, we look at the consequences for adult wages of early job stability or churning measured by the number of jobs or number of one-digit industries or occupations of employment in the five-year post-schooling period. For both men and women, whether or not we control for tenure, the estimated effect of number of jobs is either negative or zero. For men, the negative effects are generally significant in the specifications excluding tenure. Again, the combined results imply no effect of job churning conditional on job tenure as an adult, but the partial correlations revealed by the regressions imply that adult tenure, which is positively associated with wages, tends to be lower for those who held many jobs during their first years in the labor market. The same is true when we look at number of occupations, as well as number of industries for men. For women, though, there is a positive effect of number of industries that is significant in most of the specifications, indicating that wages are higher by two to three percent for each additional industry worked in during the five-year post-schooling period.[29]

Sibling Estimates

In the preceding section, we used the AFQT score and the first and last wages to control for individual heterogeneity. A different approach to reducing or eliminating heterogeneity bias is to assume that the unobservable characteristics that are correlated with other regressors are specific to families. For this method to be valid, we must assume that although an individual-specific component of the unobservable attributes may exist, it is uncorrelated with the other regressors, or that only family-specific unobserved characteristics influence individual early labor market

[29]It is possible that some job churning is useful, but that too much is a bad thing. We explored this possibility by including linear and quadratic terms in the number of jobs or number of industries or occupations in each of these specifications. The estimated coefficients of the quadratic terms were never significant, and the qualitative results were unchanged.

experiences as well as the later wage.[30] As an example, a family "work ethic" may induce siblings to self-select into a training program at a higher rate than siblings from other families. The estimated return to participation in this training program may be upward biased, as the unobserved work ethic that affects labor market success independently of the training program also influences participation in the training program. Relying on this assumption, we match the NLSY household identification codes to create a sibling sub-sample, and control for within-family effects by differencing the wage equation across siblings. The regression coefficients then identify the effects from differences across siblings in early labor market experiences. To obtain a larger sample, we shortened the five-year post-schooling window to a three-year post-schooling window for this analysis only.[31,32] The matched sibling sample for the three-year post-schooling period contains 537 respondents of which 284 are males and 253 are females.

Table 8 presents the OLS and fixed family effects estimates for males, for the early labor market variables included jointly, with and without tenure included in the regression. In all specifications, the effect of the starting industry wage on the first job is negative and significant at the five-percent level. In the full sample the estimated coefficient of the starting industry wage was insignificant, and generally negative, although not always. The within-family estimate is either the same as or slightly lower than the cross-section estimate. Because there is no difference between the OLS and within-family estimates, we conclude that the significant effect of the starting industry wage is attributable to the sample, and not to the removal of heterogeneity bias. Therefore, with respect to the starting industry wage we put more weight on the full-sample results.

Although insignificant in the full sample, the estimated coefficient of the length of training variable is significant (and negative) in the OLS sibling sample regression in both the non-tenure and tenure specifications.

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[30]If this is not the case, it is possible that using within-family data may cause the bias to increase. For a more detailed discussion of the issues involved with using sibling data, see Griliches (1979).

[31]With the five-year window, the matched sample contained only 273 respondents. To study the effect of going to a three-year window, we first checked whether the wage regression estimates for the three-year post-schooling sample were similar to those for the five-year sample. The base wage regression estimates were very similar. There were, however, a few differences in the estimated coefficients of the early labor market experience variables. For men, the estimate for on-the-job training, which was insignificant in the five-year sample, was positive and significant in the three-year sample. For women, the return to working in an additional industry was no longer significant in the three-year sample. Overall, though, most of the qualitative results for the full sample are little changed by shortening the post-schooling horizon to three years.

[32]Generally, the means for the matched sibling sample were similar to the means of the unmatched three-year sample. The estimated coefficients of the base wage regression for the matched sibling sample were also fairly similar to those for the full three-year sample, although there were some differences. First, the estimated coefficients of the quadratic experience (and tenure) variables were generally not significant, so these variables were dropped. Second, the estimated return to each year of schooling was only about five percent for males, which is a bit lower than estimates from either the three- and five-year samples.

However, the effect of length of training becomes insignificant in the within-family estimates. The estimated effect of training spells is positive but insignificant in the OLS regressions, but positive and significant (marginally so, when tenure is included) in the within-family estimates. Of the different types of training, off-the-job has the most consistently positive effects. The differences between the OLS and within-family estimates indicate that cross-section estimates of the effects of on-the-job training are biased upward because of individual heterogeneity, while cross-section estimates of the effects of off-the-job training are biased, if anything, downward. Overall, the within-family estimates lead to higher estimates of the returns to training.

In Table 9, the standard OLS and within-family fixed family effects estimates are reported for females. Although the estimated effects of on-the-job training spells are positive and significant in all specifications in the OLS estimates for the sibling sample, the estimated return from an additional spell of on-the-job training is insignificant when account is taken of within-family unobservables. This may indicate that the females participating in on-the-job training possess an unobservable characteristic that is common across siblings and is also positively associated with later wages.

In contrast to all of the full-sample results, the estimated effect of the starting wage on the occupation of the first job in the fixed family effects specification is positive and significant (at about three percent), for both the tenure and non-tenure specifications. In addition, staying in the same occupation of the first job boosts the current wage by between 18 and 21 percent when within-family effects are accounted for in the non-tenure specifications. When tenure is included in the regression, the estimated return to staying in the same occupation falls by two percent. Thus, once within-family unobservables are accounted for, it seems that those women who either start in a high-wage occupation or remain in the occupation of the first job eventually earn more.

Overall, then, while caution should be exercised in generalizing the estimates based on the sibling sample to the larger population, a couple of findings do emerge. For men, the results for the full sample suggested that training is probably the most important determinant of higher wages as an adult, and other early labor market experiences had little consequence. The within-family estimates tend to reinforce that result. For women, in contrast, the within-family estimates--in contrast to the OLS estimates for the full sample as well as the sibling sample--do not point to significant effects of on-the-job training, but do point to lasting effects of beginning work in a higher-wage occupation, and of remaining in one's first occupation. However, we caution that our within-family estimates of the returns to training and other early labor market experiences are the first of which we are aware (in contrast to the large literature on

sibling estimates of the return to schooling), and we think that further research along these lines is needed to better solidify and interpret such evidence.

Benefits

Wages are not the only dimension along which to measure labor market success. The NLSY provides data on a wide variety of benefits offered by employers. We focus on the two that are probably of by far the greatest value: health insurance and pensions. Benefit data are collected from all but the unincorporated self-employed and those working fewer than 20 hours per week, so we exclude these individuals from the analysis, hence restricting ourselves largely to full-time wage and salary workers.[33]

In Table 10, we report probit estimates for the provision of health insurance. We report results using the control variables from the earlier wage equations, and including the early labor market variables jointly. For men, but not for women, the number of training spells is significantly positively related to the provision of health insurance.[34] On the other hand, for women but not for men, length of training is negatively related to the provision of this benefit. In addition, for men both the number of community college spells, and longest job tenure in the early years (when we do not control for current tenure) are significantly positively associated with health insurance. Finally, for women only, remaining in the same occupation has a significant positive effect on the probability of receiving health insurance.[35]

Next, we look at similar questions regarding the provision of retirement plans. Table 11 reports the estimated coefficients of the early labor market variables in similar probits. For men, the number of training spells is positively related to the availability of retirement plans. The estimated effects of the length of training variables, however, are negative and marginally significant for both men and women. For men, remaining in the same occupation is significantly negatively related to the availability of a retirement plan. Finally, for women, the number of jobs held in the early labor market years is negatively related to the availability of such a plan.

In our view, the analysis of benefits parallels the results for wages in failing to provide consistent evidence

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[33]In general, the evidence we report in this section linking early labor market experiences to benefits was weaker if we included part-time and unincorporated self-employed workers, coding them as not receiving these benefits.

[34]In estimations in which types of training are distinguished, it is off-the-job training that is significantly positively associated with retirement plans, paralleling the results for wages. The same is also true for health insurance, discussed below.

[35]For both health and pension benefits, the results were very similar when we included the early labor market variables individually in separate estimations.

linking early job stability and attachment to the labor market to the receipt of pension and health benefits. This is especially true for men (although for them, training increases the likelihood of receiving such benefits), while for women there is perhaps stronger evidence suggesting that early stability and attachment is more likely to lead to such benefits.

VI. Conclusions

This paper has three goals: 1) to document and characterize early labor market experiences of men and women in the U.S. economy, 2) to explore the evolution of these labor market experiences over the first five years in the labor market, and 3) to study the relationships between these early labor market experiences and adult labor market outcomes. The overriding goal of the empirical analysis is to shed light on the consequences of initial periods of "churning," "floundering about," or "mobility" in the labor market, to help assess whether faster transitions to stable employment relationships would be likely to lead to better labor market outcomes.

Unfortunately, we do not have data generated from random assignment of individuals to faster or slower school-to-work transitions. Consequently, we have to resort to documenting numerous empirical relationships, exercising caution in using the results to draw causal inferences. Nonetheless, we think the most likely "error" to come out of such an analysis is to find evidence that individuals with early job market stability have better adult labor market outcomes, and then to conclude that policies to increase this stability would improve labor market outcomes for other individuals. The problem with such an inference would be that good job matches result in both early stability and better adult outcomes, and any policy efforts to increase stability would have to act through creating good job matches, rather than increasing stability per se.

However, our interpretation of the large set of results that we present is that there is at best modest evidence linking early job market stability to better labor market outcomes. First, we find that labor market outcomes toward the end of the five-year post-schooling period that we use are not driven very strongly by what happens in the first year or two in the labor market. In particular, once an individual switches to stable employment or a stable job for a period of a year, that individual is nearly as likely to continue with stable employment or a stable job in the following years as is someone who exhibited this stability immediately upon entering the labor market. Thus, in terms of the shorter-run "transition" to stable employment, the experiences of the first year or two are not decisive. However, training in the earliest years is positively related to later training. Second, we find that adult labor market outcomes (defined as of the late 20s or early to mid-30s) are for the most part unrelated to early labor market experiences, especially for men, with

the important exception of training. For women, in contrast, some of the evidence suggests that job stability and initial entry into a high-wage occupation has beneficial effects. This difference between the sexes may arise because women need to signal their attachment to the labor market when they are young in order to acquire jobs with high wages and generous benefits as adults. For this reason, and others discussed earlier, any positive effects of early job stability that we do find probably reflect upper bound estimates of the causal effects.

Based on this evidence, we do not see a compelling case for efforts to explicitly target the school-to-work transition, insofar as this implies changing the structure of youth labor markets so that young persons become more firmly attached to an employer, or an industry or occupation, at younger ages. Of course there may be information problems in the labor market, so that improving young persons' information about labor market opportunities would be helpful. Such efforts might include incorporation of work experience with schooling, or greater communication and interaction between employers and schools. Furthermore, the relatively consistent evidence we find that early training helps workers might be used to support efforts to increase the amounts of training young workers receive. However, more on-the-job training, to the extent that the training is specific or employer-financed, may only come about with the expectation of greater job stability. But given that there is some evidence that, for men at least, increased early job stability (in the form of remaining in the first industry or occupation) has some costs, we must be cautious in assuming that policies that increase training and stability in concert represent unambiguous improvements.

Finally, our empirical analysis examines the effects of early labor market experiences in the jobs in which young workers presently tend to enter the labor market. Our results would not be generalizable to school-to-work programs that substantially alter the types of jobs held by new labor market entrants. At this point, though, we have uncovered relatively little evidence suggesting that youth labor market policies that result in lower mobility of young workers would lead to better adult labor market outcomes.

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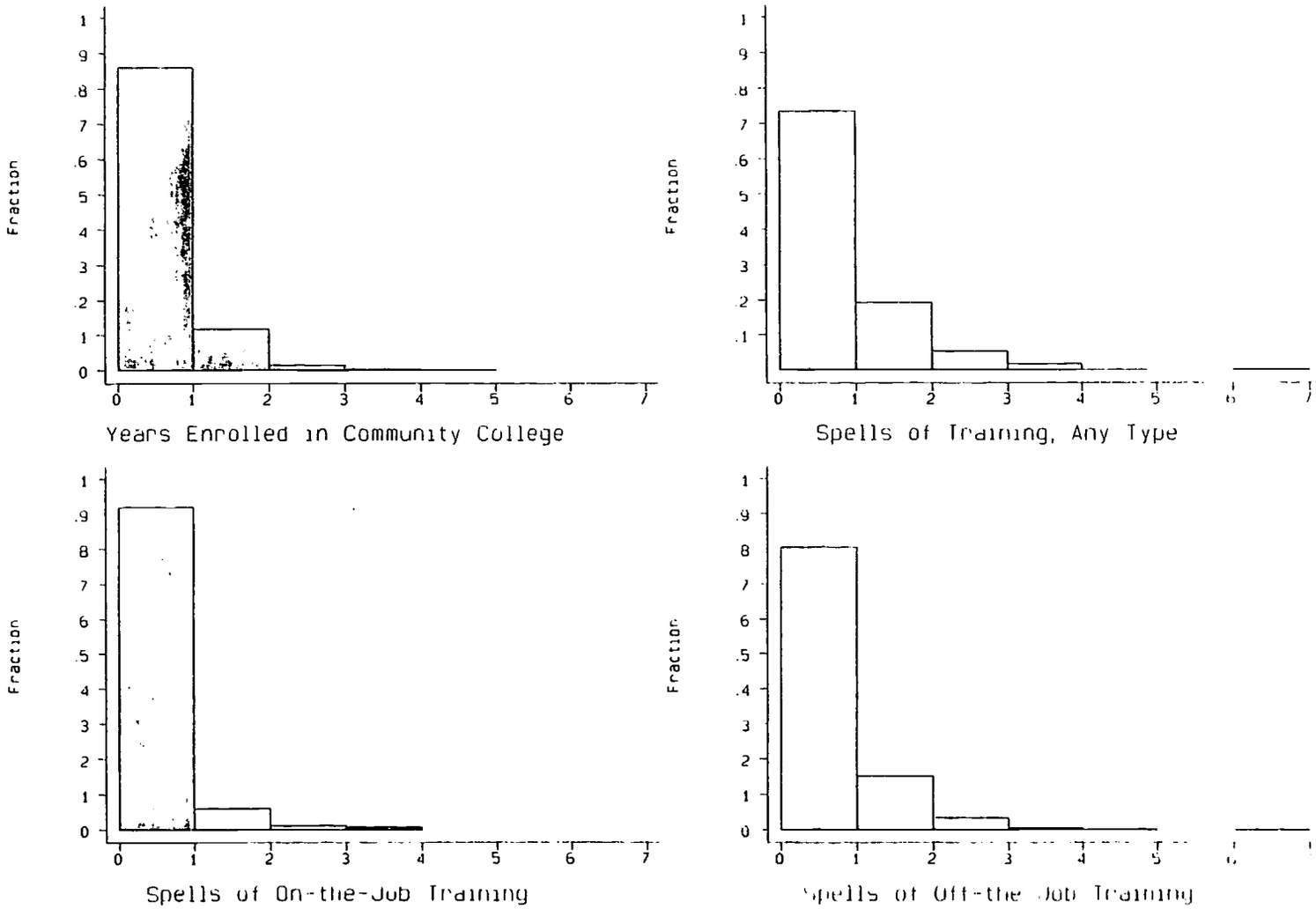
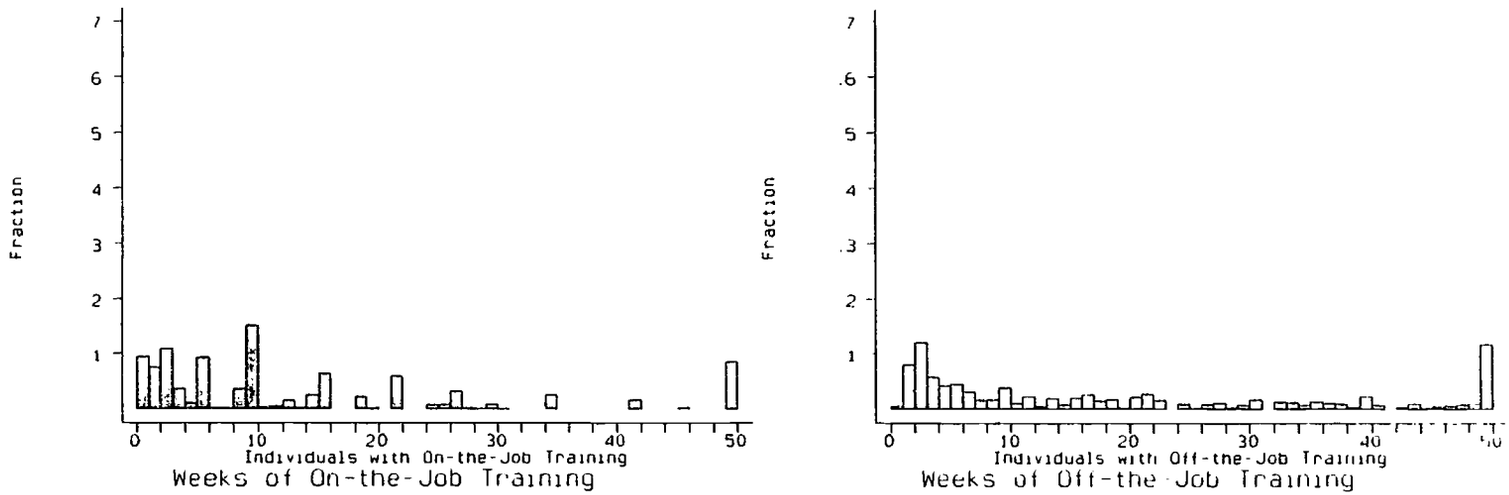


Figure 1: Post Schooling Community College/Training



Max.=50 Weeks

Figure 2: Weeks During the Post-Schooling Period

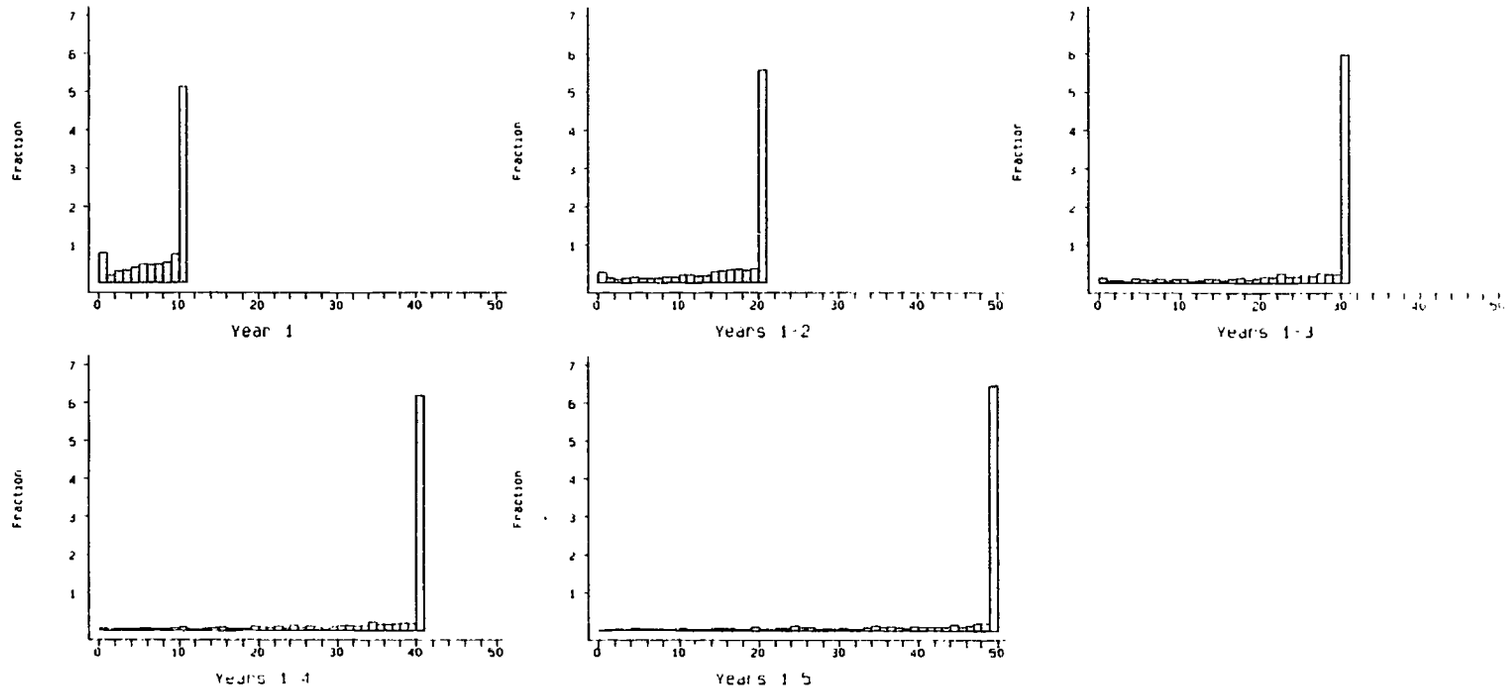


Figure 3: Months of Work

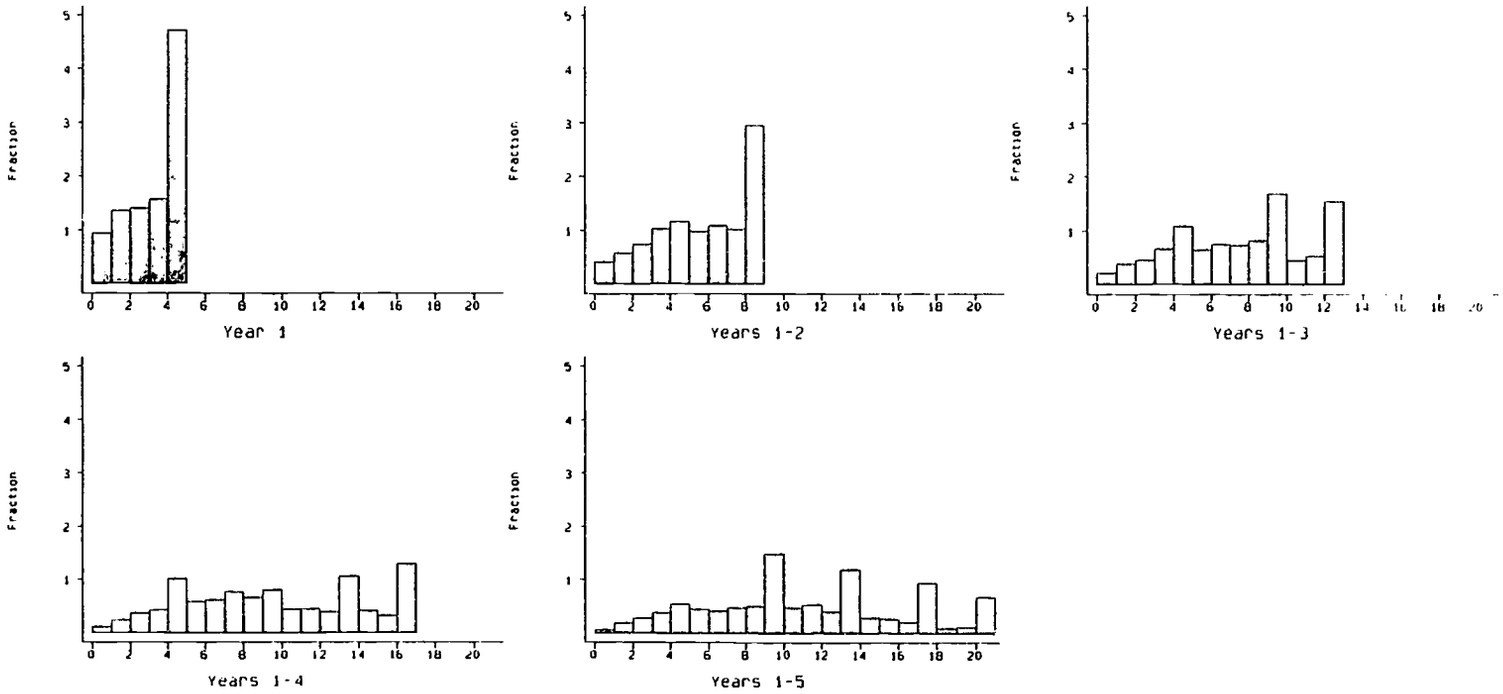


Figure 4: Longest Job (Quarters)

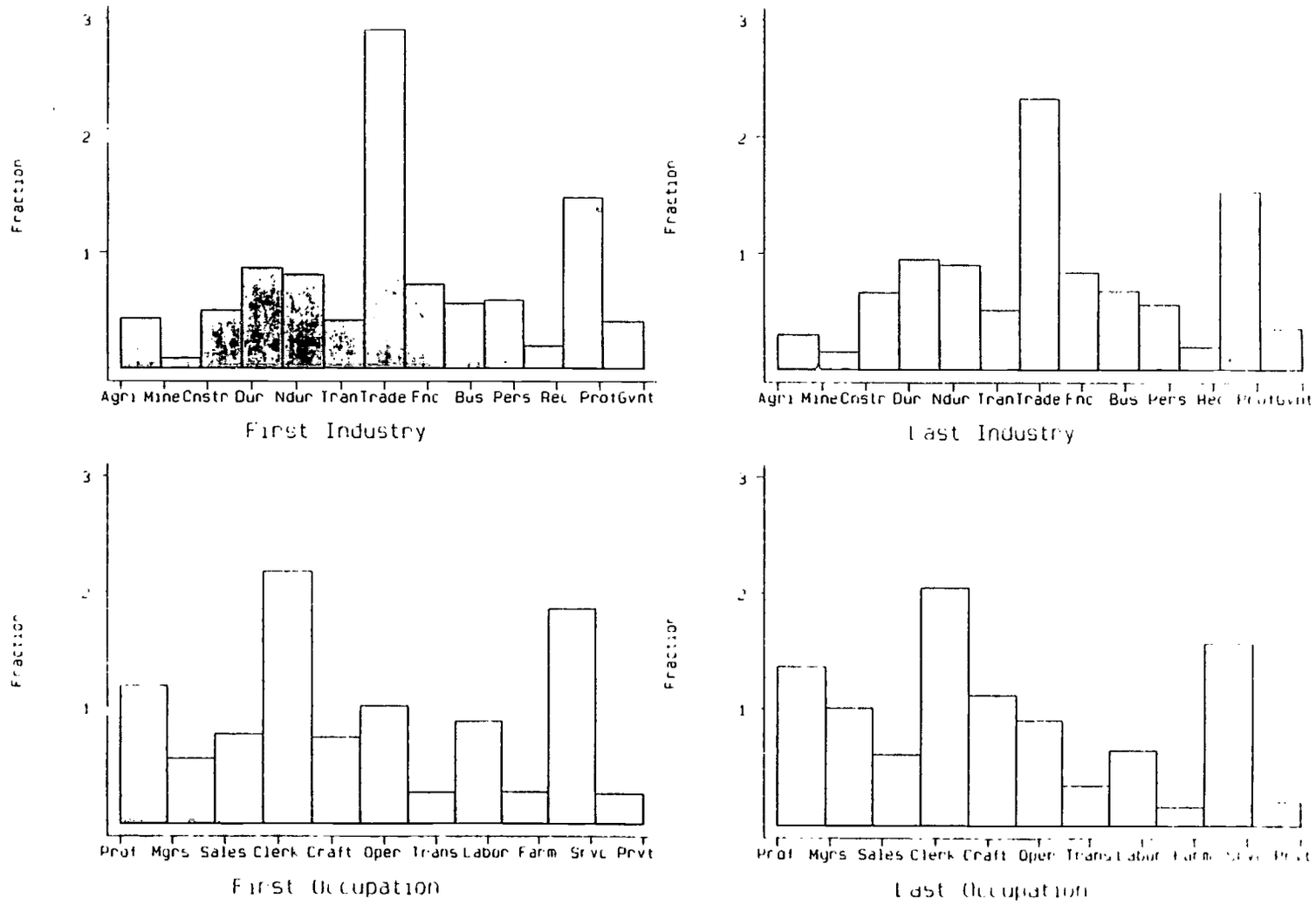


Figure 5: First Schooling Industries and Occupations

Table 1: Probabilities of Additional Years of Any Training, Based on Any Training in First Year

	Probabilities of additional:		
	No years	One year	Two years
Training in first year (12%)	.77	.21	.02
No training in first year (88%)	.90	.09	.01

Estimates are based on weighted data. Probabilities are calculated over first three post-schooling years. Probabilities do not always add to one due to rounding.

Table 2: Probabilities of Additional Years of Full-Year (≥ 9 Months) Work, Based on Full-Year Work in First Year and First Two Years

	Probabilities of additional:				
	No years	One year	Two years	Three years	Four years
A. Full-year work in first year (55%)	.03	.05	.08	.21	.63
No full-year work in first year (45%)	.19	.15	.14	.19	.33
B. Full-year work in first and second year	.02	.06	.18	.75	
No full-year work in first year, full-year work in second year	.08	.09	.19	.64	
No full-year work in first and second year	.39	.22	.19	.20	

Estimates are based on weighted data. Probabilities are calculated over first five post-schooling years. Probabilities do not always add to one due to rounding.

Table 3: Probabilities of Additional Years with Tenure ≥ 4 Quarters, Based on Tenure in First Year and First Two Years

	Probabilities of additional:				
	No years	One year	Two years	Three years	Four years
A. Tenure ≥ 4 in first year (47%)	.02	.06	.09	.17	.66
Tenure < 4 in first year (53%)	.20	.12	.18	.19	.30
B. Tenure ≥ 4 in first and second year	.04	.06	.12	.79	
Tenure < 4 in first year, ≥ 4 in second year	.03	.09	.13	.74	
Tenure < 4 in first and second year	.34	.17	.24	.24	

Estimates are based on weighted data. Probabilities are calculated over first five post-schooling years. Probabilities do not always add to one due to rounding.

Table 4: Probit Estimates for Full-Year Work in Last Post-Schooling Year

				Fourth Post-Schooling Year
	(1)	(2)	(3)	(4)
Worked ≥ 9 months in year 1	.27 (.02)	.07 (.02)	.06 (.02)	09 (.02)
Worked ≥ 9 months in year 2	...	.14 (.02)	.12 (.03)	.10 (.03)
Worked ≥ 9 months in year 3	...	.11 (.03)	.10 (.03)	.43 (.02)
Worked ≥ 9 months in year 4	...	.47 (.02)	.44 (.03)	...
Non-white	...	...	-.01 (.03)	-.01 (.03)
Female	...	...	-.16 (.02)	-.17 (.02)
AFQT residual	...	...	.002 (.001)	.004 (.001)
Years of education	...	...	.0126 (.0076)	.02 (.01)
Spells of community college	...	...	-.004 (.03)	.02 (.03)
Region and SMSA dummy variables included	No	No	Yes	Yes

Model is estimated for probability of full-year work. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives, are reported. Sample weights are not used. A constant and dummy variables for the year of the observation are included in all specifications. In columns (1)-(3) the dependent variable is defined for the fifth post-schooling year. In column (4), the dependent variable is defined for the fourth post-schooling year. There are 2442 observations.

Table 5: Probit Estimates for High-Tenure Job in Last Post-Schooling Year

				Fourth Post-Schooling Year
	(1)	(2)	(3)	(4)
Tenure ≥ 4 quarters in year 1	.24 (.02)	.09 (.03)	.07 (.03)	.07 (.03)
Tenure ≥ 4 quarters in year 2	...	.11 (.03)	.09 (.03)	.04 (.03)
Tenure ≥ 4 quarters in year 3	...	.05 (.03)	.03 (.03)	.51 (.03)
Tenure ≥ 4 quarters in year 4	...	.55 (.03)	.53 (.03)	...
Non-white	...	...	-.01 (.03)	-.06 (.03)
Female	...	...	-.12 (.02)	-.11 (.02)
AFQT residual	...	...	.003 (.001)	.002 (.001)
Years of education	...	...	.02 (.01)	.04 (.01)
Spells of community college	...	...	.03 (.03)	.003 (.03)
Region and SMSA dummy variables included	No	No	Yes	Yes

Model is estimated for probability of four or more quarters of tenure. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives, are reported. Sample weights are not used. A constant and dummy variables for the year of the observation are included in all specifications. In columns (1)-(3) the dependent variable is defined for the fifth post-schooling year. In column (4), the dependent variable is defined for the fourth post-schooling year. There are 2442 observations.

Table 6: Wage Regression Estimates, Males

	Variables Entered Individually, Excl. Tenure					Variables Entered Jointly, Excl. Tenure		Variables Entered Jointly, Incl. Tenure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Training Spells:	.08	.06	.06	.06	.07	.11	.09	.09	.09
	(.02)	(.02)	(.02)	(.02)	(.03)	(.03)	(.03)	(.02)	(.03)
By Type:									
On-the-Job	.06	.04	.02	.02	.02			.06	
	(.05)	(.05)	(.05)	(.05)	(.06)			(.05)	
Off-the-Job	.08	.06	.06	.06	.07			.09	
	(.03)	(.03)	(.03)	(.03)	(.03)			(.03)	
Apprenticeship	.31	.26	.24	.15	.06			.27	
	(.12)	(.12)	(.11)	(.11)	(.19)			(.12)	
Training Length:	.03	.002	.002	.01	-.001	-.09	-.09	.04	-.09
	(.05)	(.05)	(.05)	(.05)	(.06)	(.06)	(.06)	(.05)	(.06)
By Type:									
On-the-Job	-.02	-.05	-.04	-.01	.01			-.002	
	(.11)	(.11)	(.10)	(.10)	(.11)			(.11)	
Off-the-Job	.0-	.01	.02	.01	-.02			.05	
	(.0-	(.06)	(.06)	(.06)	(.08)			(.06)	
Apprenticeship	.10	.08	.02	.02	.07			.11	
	(.17)	(.17)	(.16)	(.16)	(.17)			(.17)	
Community College Spells:	-.02	-.03	-.02	-.02	-.04	-.01	-.04	-.03	-.05
	(.04)	(.04)	(.03)	(.03)	(.04)	(.04)	(.04)	(.04)	(.04)
Longest Job Tenure:	.04	.03	.03	.02	.01	.02	.002	.02	.004
	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)	(.01)	(.02)
Ind./Occ. of First Job:									
Same Industry	-.01	-.01	-.02	-.04	-.09	-.02	-.09	-.05	-.09
	(.03)	(.03)	(.03)	(.03)	(.04)	(.03)	(.04)	(.03)	(.04)
Same Occupation	-.05	-.045	-.06	-.046	-.03	-.06	-.04	-.06	-.04
	(.03)	(.033)	(.03)	(.032)	(.04)	(.03)	(.04)	(.03)	(.04)
Starting Wage on First Job:									
Industry	-.01	-.01	-.01	.0002	.02	-.002	.02	-.01	.02
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)
Occupation	.001	.002	-.001	-.0003	.001	.001	.001	.001	.001
	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)	(.004)
Number of Jobs During First 5 Years:	-.03	-.03	-.03	-.03	-.020	-.030	-.032	-.020	-.02
	(.01)	(.01)	(.01)	(.01)	(.013)	(.017)	(.019)	(.012)	(.02)
Number of Ind./Occ. During First 5 Years:									
Industry	-.01	-.004	-.001	-.001	.01	.01	.02	-.001	.01
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.01)	(.02)
Occupation	-.005	-.01	-.001	-.001	-.0004	-.004	-.0004	.0002	.001
	(.01)	(.01)	(.01)	(.01)	(.02)	(.01)	(.02)	(.01)	(.02)
AFQT	No	Yes	Yes	Yes	Yes	No	Yes	No	Yes
First Wage	No	No	Yes	No	No	No	No	No	No
Last Wage	No	No	No	Yes	Yes	No	Yes	No	Yes
Not Same Job	No	No	No	No	Yes	No	Yes	No	Yes
N	1094	1094	1094	1094	846	1094	846	1094	846

Dependent variable is log hourly wage. Standard errors of coefficient estimates are reported in parentheses. The other control variables are given in the text. Training and tenure are measured in years. In columns (1)-(5) each set of coefficients corresponding to the underlined entries in the left-hand column is estimated from a separate wage regression. Sample weights are not used.

Table 7: Wage Regression Estimates, Females

	Variables Entered Individually, Excl. Tenure					Variables Entered Jointly, Excl. Tenure		Variables Entered Jointly, Incl. Tenure	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(1')	(7')
Training Spells:	.02	.01	.01	.01	.02	.01	.01	.02	01
	(.02)	(.02)	(.02)	(.02)	(.02)	(.02)	(.03)	(.02)	(.03)
By Type:									
On-the-Job	.11	.11	.11	.10	.12			.11	
	(.04)	(.04)	(.04)	(.04)	(.05)			(.04)	
Off-the-Job	-.004	-.01	-.01	-.01	-.01			-.00	
	(.02)	(.02)	(.02)	(.02)	(.02)			(.02)	
Apprenticeship	.03	.03	.04	-.05	.25			.05	
	(.24)	(.24)	(.24)	(.24)	(.30)			(.24)	
Training Length:	.04	.03	.03	.03	.03	.04	.03	.04	.02
	(.04)	(.04)	(.04)	(.03)	(.04)	(.04)	(.05)	(.03)	(.05)
By Type:									
On-the-Job	.26	.26	.26	.23	.26			.24	
	(.09)	(.09)	(.09)	(.09)	(.10)			(.09)	
Off-the-Job	.01	-.003	-.002	.002	-.01			.005	
	(.04)	(.04)	(.04)	(.04)	(.04)			(.04)	
Apprenticeship	.84	.77	.83	.25	1.20			1.07	
	(1.22)	(1.20)	(1.20)	(1.19)	(1.27)			(1.20)	
Community College Spells:	.04	.03	.03	.03	.04	.04	.04	.03	.03
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.04)	(.03)	(.04)
Longest Job Tenure:	.02	.01	.01	.01	-.003	.02	.002	.01	.01
	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.02)	(.01)	(.02)
Ind./Occ. of First Job:									
Same Industry	.01	.01	.004	.002	.01	.02	.02	-.004	.02
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Same Occupation	-.004	-.003	-.01	-.01	-.01	-.01	-.02	-.01	-.02
	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)	(.03)
Starting Wage on First Job:									
Industry	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.01	-.01
	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)	(.01)
Occupation	-.006	-.005	-.004	-.004	-.004	-.006	-.004	-.006	-.004
	(.004)	(.004)	(.004)	(.004)	(.005)	(.004)	(.005)	(.004)	(.005)
Number of Jobs During First 5 Years:	-.01	-.01	-.01	-.002	.01	-.01	-.002	.01	.01
	(.01)	(.01)	(.01)	(.01)	(.01)	(.02)	(.02)	(.01)	(.02)
Number of Ind./Occ. During First 5 Years:									
Industry	.02	.02	.02	.02	.03	.035	.03	.03	.03
	(.01)	(.01)	(.01)	(.01)	(.02)	(.015)	(.02)	(.01)	(.02)
Occupation	-.01	-.01	-.01	-.01	-.02	-.01	-.01	-.01	-.01
	(.01)	(.01)	(.01)	(.01)	(.02)	(.01)	(.02)	(.01)	(.02)
AFQT	No	Yes	Yes	Yes	Yes	No	Yes	No	Yes
First Wage	No	No	Yes	No	No	No	No	No	No
Last Wage	No	No	No	Yes	Yes	No	Yes	No	Yes
Not Same Job	No	No	No	No	Yes	No	Yes	No	Yes
N	1059	1059	1059	1059	848	1059	848	1059	848

Dependent variable is log hourly wage. Standard errors of coefficient estimates are reported in parentheses. The other control variables are given in the text. Training and tenure are measured in years. In columns (1)-(5) each set of coefficients corresponding to the underlined entries in the left-hand column is estimated from a separate wage regression. Sample weights are not used.

Table 8: Wage Regression Estimates, Males, Sibling Sample

	Excluding Tenure, Ordinary Least Squares	Excluding Tenure, Fixed Effects	Including Tenure, Ordinary Least Squares	Including Tenure Fixed Effects
	(1)	(2)	(3)	(4)
Training Spells:	.32	.18	.29	.17
	(.09)	(.11)	(.09)	(.11)
Training Length:	-.98	-.24	-.89	-.22
	(.25)	(.31)	(.25)	(.31)
Community College Spells:	.03	-.02	.03	-.03
	(.08)	(.09)	(.08)	(.09)
Longest Job Tenure:	.02	.08	.01	.08
	(.05)	(.06)	(.05)	(.06)
Ind./Occ. of First Job:				
Same Industry	-.04	-.02	-.07	-.04
	(.07)	(.08)	(.07)	(.08)
Same Occupation	-.08	-.118	-.09	-.13
	(.07)	(.072)	(.07)	(.07)
Starting Wage on First Job:				
Industry	-.07	-.07	-.07	-.07
	(.02)	(.03)	(.02)	(.03)
Occupation	.0001	.004	-.001	.003
	(.01)	(.01)	(.01)	(.01)
Number of Jobs During First 3 Years:	.004	.02	.01	.02
	(.06)	(.07)	(.06)	(.07)
Number of Ind./Occ. During First 3 Years:				
Industry	-.07	-.08	-.07	-.08
	(.05)	(.06)	(.05)	(.06)
Occupation	-.05	-.04	-.05	-.05
	(.05)	(.05)	(.05)	(.05)
N	284	284	284	284

Dependent variable is log hourly wage. Standard errors of coefficient estimates are reported in parentheses. The other control variables are given in the text. Training and tenure are measured in years. In all specifications, the early labor market experience variables are entered jointly. Sample weights are not used.

Table 9: Wage Regression Estimates, Females, Sibling Sample

	Excluding Tenure, Ordinary Least Squares	Excluding Tenure, Fixed Effects	Including Tenure, Ordinary Least Squares	Including Tenure, Fixed Effects
	(1)	(2)	(3)	(4)
Training Spells:	.06	-.003	.07	.01
	(.07)	(.09)	(.06)	(.09)
Training Length:	-.02	-.03	-.03	-.01
	(.11)	(.16)	(.11)	(.16)
Community College Spells:	-.02	.09	-.01	.09
	(.08)	(.11)	(.07)	(.10)
Longest Job Tenure:	.004	-.01	.01	-.01
	(.05)	(.07)	(.04)	(.07)
Ind./Occ. of First Job:				
Same Industry	.05	.06	-.03	-.02
	(.07)	(.09)	(.06)	(.09)
Same Occupation	.11	.21	.07	.19
	(.06)	(.09)	(.06)	(.09)
Starting Wage on First Job:				
Industry	.01	.04	.01	.03
	(.02)	(.03)	(.02)	(.03)
Occupation	.01	.04	.004	.04
	(.01)	(.02)	(.01)	(.01)
Number of Jobs During First 3 Years:	-.03	-.11	-.01	-.07
	(.06)	(.08)	(.06)	(.08)
Number of Ind./Occ. During First 3 Years:				
Industry	.04	.06	.02	.05
	(.05)	(.07)	(.05)	(.06)
Occupation	.07	.14	.05	.13
	(.05)	(.08)	(.05)	(.07)
N	253	253	253	253

Dependent variable is log hourly wage. Standard errors of coefficient estimates are reported in parentheses. The other control variables are given in the text. Training and tenure are measured in years. In all specifications, the early labor market experience variables are entered jointly. Sample weights are not used.

Table 10: Probability of Working at a Job That Provides Health Insurance

	Health Insurance Probit: Males	Health Insurance Probit: Females	Health Insurance Probit: Males	Health Insurance Probit: Females
	(1)	(2)	(3)	(4)
Training Spells:	.07	.04	.07	04
	(.03)	(.03)	(.03)	(.03)
Training Length:	-.05	-.09	-.03	-.10
	(.06)	(.05)	(.06)	(.05)
Community College Spells:	.11	.04	.11	.03
	(.04)	(.04)	(.04)	(.04)
Longest Job Tenure:	.04	-.01	.029	-.01
	(.02)	(.02)	(.018)	(.02)
Ind./Occ. of First Job:				
Same Industry	.001	-.04	-.03	-.06
	(.04)	(.04)	(.04)	(.04)
Same Occupation	-.04	.06	-.05	.06
	(.03)	(.03)	(.03)	(.03)
Starting Wage on First Job:				
Industry	.02	-.01	.03	-.01
	(.02)	(.01)	(.02)	(.01)
Occupation	.0002	-.002	.001	-.002
	(.004)	(.01)	(.004)	(.01)
Number of Jobs in First 5 Years:	-.003	-.03	.02	-.02
	(.02)	(.02)	(.02)	(.02)
Number of Ind./Occ. During First 5 Years:				
Industry	-.01	.003	-.01	.001
	(.01)	(.02)	(.01)	(.02)
Occupation	.01	-.01	-.01	-.01
	(.01)	(.02)	(.01)	(.02)

There are 965 males and 903 females. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives, are reported. Sample weights are not used This sample excluded the unincorporated self-employed and those working less than 20 hours per week, for whom data on benefits are not reported. The variables were included jointly in all specifications. The control variables correspond to those in column (6) of Tables 6 and 7.

Table 11: Probability of Working at a Job That Provides Retirement Plan

	Retirement Plan Probit: Males	Retirement Plan Probit: Females	Retirement Plan Probit: Males	Retirement Plan Probit: Females
	(1)	(2)	(3)	(4)
Training Spells:	.11	.04	.12	.04
	(.04)	(.03)	(.04)	(.03)
Training Length:	-.11	-.11	-.10	-.11
	(.07)	(.06)	(.07)	(.06)
Community College Spells:	.01	.06	-.01	.06
	(.04)	(.04)	(.04)	(.04)
Longest Job Tenure:	.03	.01	.01	.01
	(.02)	(.02)	(.02)	(.02)
Ind./Occ. of First Job:				
Same Industry	.03	-.03	.01	-.04
	(.04)	(.04)	(.04)	(.04)
Same Occupation	-.07	.04	-.08	.03
	(.04)	(.04)	(.04)	(.04)
Starting Wage on First Job:				
Industry	-.01	.01	-.01	.01
	(.02)	(.01)	(.02)	(.01)
Occupation	.001	-.003	.002	-.003
	(.004)	(.01)	(.01)	(.01)
Number of Jobs in First 5 Years:	-.01	-.08	-.01	-.07
	(.02)	(.03)	(.03)	(.03)
Number of Ind./Occ. During First 5 Years:				
Industry	-.01	.03	-.02	.03
	(.02)	(.02)	(.02)	(.02)
Occupation	-.01	.001	-.01	.005
	(.02)	(.02)	(.02)	(.02)

There are 951 males and 879 females. Partial derivatives of probability of outcome associated with dependent variable with respect to independent variables, and standard errors of these estimated derivatives, are reported. Sample weights are not used. Restricted sample excludes the unincorporated self-employed and those working less than 20 hours per week, for whom data on benefits are not reported. The variables were included jointly in all specifications. The control variables correspond to those in column (6) of Tables 6 and 7.

Appendix Table A1: Means Across Demographic Groups

	% With Community College (1)	% With Any Training (2)	% With On-the-Job Training (3)	% With Off-the-Job Training (4)	Months of Labor Market Experience (5)	Weeks of Tenure Longest Job (6)
Male	11.93	25.95	7.97	18.76	52.76	135.12
	(32.43)	(43.85)	(27.09)	(39.06)	(14.50)	(62.82)
Female	15.65	26.99	7.78	20.00	45.84	120.31
	(36.34)	(44.41)	(26.80)	(40.01)	(18.45)	(63.87)
White	13.85	26.90	8.33	19.31	50.61	131.15
	(34.55)	(44.35)	(27.64)	(39.49)	(16.04)	(63.05)
Non-White	14.20	24.13	5.11	20.05	40.09	104.18
	(34.93)	(42.81)	(22.04)	(40.06)	(19.97)	(63.39)
≤H.S. Education	5.97	25.67	5.15	21.44	44.06	107.31
	(23.70)	(43.69)	(22.11)	(41.05)	(18.93)	(61.25)
>H.S. Education	22.67	27.42	10.88	17.18	54.69	149.40
	(41.91)	(44.65)	(31.16)	(37.75)	(12.55)	(59.12)
Long-Tenure	15.80	20.70	8.50	12.64	36.86	154.84
	(36.52)	(40.57)	(27.92)	(33.28)	(24.43)	(97.85)
Short-Tenure	13.62	27.35	7.78	20.41	50.89	123.27
	(34.31)	(44.58)	(26.79)	(40.31)	(14.88)	(56.06)
Received Training	15.01				52.04	126.67
	(35.75)				(15.36)	(64.23)
No Training Received	13.50				48.04	129.01
	(34.18)				(17.51)	(62.56)
Overall Mean	13.90	26.50	7.87	19.42	49.10	127.29
	(34.60)	(44.14)	(26.93)	(39.56)	(17.06)	(63.79)

The long-tenure variable refers to those who hold one job over the five-year post-schooling period. The short-tenure variable refers to those who hold more than one job over this period. Numbers in parentheses are standard deviations. Estimates are based on weighted data.

Appendix Table A2: Means of Training Variables Across Demographic Groups

	Spells of Community College (1)	Weeks of Training (2)	Weeks of On-the-Job Training (3)	Weeks of Off-the-Job Training (4)
Male	1.21	15.41	13.22	14.82
	(.50)	(29.57)	(25.19)	(20.06)
Female	1.17	22.41	20.47	22.09
	(.49)	(35.84)	(47.97)	(29.42)
White	1.17	19.48	17.66	18.95
	(.48)	(32.88)	(40.57)	(26.28)
Non-White	1.25	17.18	10.71	17.78
	(.55)	(22.22)	(12.96)	(23.07)
≤H.S. Education	1.24	18.56	11.21	19.14
	(.51)	(28.06)	(28.45)	(24.68)
>H.S. Education	1.17	19.81	20.05	18.29
	(.49)	(35.13)	(43.31)	(27.41)
Long-Tenure	1.01	38.45	52.50	27.66
	(.08)	(58.50)	(85.83)	(28.70)
Short-Tenure	1.21	17.05	11.36	17.98
	(.53)	(26.41)	(20.98)	(25.42)
Overall Mean	1.18	19.18	17.01	18.78
	(.49)	(31.69)	(38.85)	(25.82)

Means for spell variables are computed over those reporting at least one spell. The long-tenure variable refers to those who hold one job over the five-year post-schooling period. The short-tenure variable refers to those who hold more than one job over this period. Numbers in parentheses are standard deviations. Estimates are based on weighted data.